15/12/51

Dear Dr.Hayes,

Thank you very much for your letter of Dec.2nd ; your discovery of the retention of fertilizing ability after streptomycin sterilization is very exciting and throws much light on my present work. Thank you for letting me know it in advance of publication.

I have been doing a good deal of work with streptomycin lately, and, as you will see, some of my results are complementary to yours in a remarkable way. I shall tell you about them, as you may be interested to see what sort of confirmation through an entirely independent approach my results can bring to yours. I am not thinking of publishing this soon, as I should like to be able to conclude on one main point before - whether maps are linear or not, and unless presently conducted experiments will show that I am still very far from it ; however, you can make what use you like of this matter of mine.

Trying to follow up one of my aims - that of making strains which , in crossing, would not be heterozygous for chromosome mutations, as I think I have shown the cross 58-161 x W 677 is - I have prepared a 5 -161 with multiple sugar deficiencies (# 147) and, later, the following four strains : 58-161 S^r; 5 -161 reverted to prototrophism by back mutation and selection on minimal ; 147 S^r ; 147 reverted to prototrophism. The crosses 147 S^r x 58-161/ prototrophic and 147 prototrophic x 58-161 S^r are both successful on minimal plus streptomycin; they are complementary heterozygous for a number of sugars apparently without chromosome mu-

tations disturbing the segregations (I am not very sure yet about this last point).

On the other hand, when I tried to repeat entirely similar experiments with W 677 (making W 677 S^r and W 677 prototrophic, and the same on a variety of other strains, all having in common with W 677 the T-L-B_1- markers), the cross TLB_1-S^r x TLB_1+ on minimal + streptomycin never gave rise to any prototroph, although several independent strains were tested. Therefore I concluded that while 58-161, as the original K 12, behaved as homothallic, i.e. could forme male and female gametes, strain W 677 and all other TLB_1- derivatives behaved as heterothallic., i.e. could form only one type of gamete (perhaps the female ones after your results).

However homothallism can be restored by recombination. In fact, recombinants from 58-161 and W 677 with the W 677 phenotype can cross to W 677, and I was just following this up to control the inheritance of this mating-type-like behaviour.

I shall keep you informed of future developments, because, although our roads are not the same, they seem to me to be closely parallel.

As to Proteus, I feel I ought to have written to you about it; but my results were rather uninteresting. The round bodies, which you described to me, were however clearly visible. I made a number of mutants, none of which seemed stable enough to provide satisfactory evidence for crossing. Also, I found a paper by Dienes giving evidence against it, after work done by isolating the round bodies and testing them for natural markers.

With all my greetings for Xmas and the New Year

Yours sincerely